

## ***Interactive comment on* “Seasonality of greenhouse gas emission factors from biomass burning in the Brazilian Cerrado” by Roland Vernooij et al.**

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Response of the authors to comments by reviewer #2 – bg-2020-86-RC1, 2020 – “Seasonality of greenhouse gas emission factors from biomass burning in the Brazilian Cerrado”

Roland Vernooij (corresponding author) on behalf of the authors:

We thank Reviewer #2 for the time and effort in assessing our manuscript, and the detailed and constructive comments. Please find below our point-to-point response to the review. The revised text and updated figures are included in the updated manuscript.

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1) Fuel amount estimated from quantifying recovery time since last fire which was derived from Landsat data. Here, the study lacks to inform the reader how this data on fuel type and fuel amount is integrated into the emission factor quantification in equ. 1 and 2, respectively.

In this study we do not use fuel amounts, and they are not included in Eq. (1) and Eq. (2). As they calculate the emission factor, they primarily depend on the ratio of the emitted carbonaceous species. Through the carbon content of the fuel (which does differ for different fuel types based on literature), this is then calculated back to a g kg<sup>-1</sup> dry fuel unit. We do not attempt an estimation of the total emissions.

2) The authors need to add respective information and they need to describe how the upscaling is done in order to analyse the spatio-temporal variation.

We have added the following clarification to section 2.5:

"The weighted average emission factor (EF) for combined cerrado vegetation types in the EESGT was calculated through Eq. (3) (supplement) in which  $n$  is the number of vegetation types,  $BA_i$  is the burned area over the years 2013 to 2018 and  $BA_{tot}$  is the burned area over the same period. Since we lack detailed fuel load and combustion completeness data, the EF for EESGT is based on  $BA$ ."

3) The results describe seasonality pattern found in emission factors for N<sub>2</sub>O, CO and CH<sub>4</sub>. The authors find that N<sub>2</sub>O has seasonality trends opposite to CO and CH<sub>4</sub>, where the latter indicate incomplete combustion. Statistical significance are mentioned, but not reported in detail with respective results in section 3.2. Even though it is marked in Table 3, examples should be provided in the text.

In the revised manuscript we now refer explicitly refer to the significance of the results in the abstract, results and discussion:

in sect 3.1: "only the slight differences in open grasslands and the 14% and 34% increases in N<sub>2</sub>O EF for open cerrado and typical cerrado, respectively, were statistically

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significant using a two-tailed t-tests with unequal variance at a 90% significance level.”

in sect 4.1: “intraseasonal variability was smaller compared to the variability within EDS or LDS campaigns, and the difference was not statistically significant ( $p < 0.1$ )

4) The results are then discussed in detail and contextualized using earlier publications, offering the reader to understand where earlier findings could be confirmed and where uncertainties, especially for N<sub>2</sub>O, still persist. It underlines the importance of reporting spatio-temporal variability in each measurement campaign also in global studies. The discussion contains a detailed description of uncertainties arising from sampling strategy, multi-day burning fires, and emission factor calculation. To avoid confusion, please also cite the original study where these numbers were taken from (it is correctly done in the methods, but worth repeating here on page 15, line 2).

We added the references to the discussion

5) p. 15, lines 14-23: The discussion of the role of peat carbon contributing to carbon combustion in Cerrado fires is somewhat arbitrary, since peat combustion was not explicitly measured in these experiment, nor was the carbon storage in organic soils quantified or its proportion in the study area quantified. I would suggest to carefully discuss the wider implications of burning organic (peat) soils in the Cerrado.

After closely examining the conditions under which peat burns, we decided that we cannot state with certainty that peat burned in the humid grassland fire we measured. Since the higher carbon content of 56% was based on this assumption, we have reduced this to 48% which is also used for the other cerrado species. We then recalculated the results leading to lower EFs for humid grasslands by  $\approx 15\%$ . This did not alter any of the main findings of the study. We have added the following text to the manuscript:

Sect. 4.4.2: ‘The carbon content in humid grasslands is based on the assumption no peat, which has a higher carbon content of  $\approx 56\%$  (Susott et al., 1996), was com-

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busted in the fire.'

Sect. 4.4.3: 'Based on our measurements, we cannot conclude whether peat from the soil underlying the humid grasslands contributed to the fuel mixture.'

6) The key finding of this study is clearly the fact that lower N<sub>2</sub>O emissions were found that could impact global N<sub>2</sub>O budgets if the burning conditions measured here are representative of all savannah areas which are a large contributor to global biomass burning. However, the conclusion should also contain key results (numbers) for the EF factors for CO, CH<sub>4</sub> and N<sub>2</sub>O, incl. their uncertainty range.

We added the following lines to the conclusion: 'WA EFs over the combined cerrado vegetation in EESGT for CO, CH<sub>4</sub> and N<sub>2</sub>O where 48 g kg<sup>-1</sup>, 0.78 g kg<sup>-1</sup> and 0.11 g kg<sup>-1</sup>, respectively in the EDS. In the LDS, WA EFs were 41 g kg<sup>-1</sup> for CO (-15% from EDS), 0.68 g kg<sup>-1</sup> for CH<sub>4</sub> (-13% from EDS) and 0.12 g kg<sup>-1</sup> for N<sub>2</sub>O (+17% from EDS). Apart from the intraseasonal N<sub>2</sub>O EF decrease in grasslands and increase in typical cerrado, we did not find major seasonal differences that were statistically significant.'

7) p. 8, line 12: please explain BA abbreviation

It refers to burned area as observed by satellite observations. We included "burned area (BA)" in the abstract

8) p.9, line 25: it should read "In Figs. 5-7 the green diamond"

We changed it in the manuscript.

9) p. 12, line 11: explain abbreviation RSC

It refers to Residual Smouldering Combustion. This is now written out the first time we refer to the abbreviation

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2020-86/bg-2020-86-AC2-supplement.pdf>

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